July 11, 1951.

Dear Dr. Käseneberger-Nobel:

I have meant to write you sooner, but this was my first opportunity since returning from the symposium at Cold Spring Harbor, which dealt in a narrow way with certain aspects of microbial genetics and cytology. However, I had the opportunity to talk with Robinow, both at some bacteriologists' meetings in Chicago, and at his home grounds at London, Ontario. We concurred in our enthusiasm and interest in your work, and in the hope that it might be possible to make the arrangements that might manage to persuade you to visit this continent, and do a bit of proselytizing for the L-forms. I am, unfortunately, not in a position to offer an invitation, but perhaps it might not be too impertinent to ask whether you would, under any circumstances consider one. There are any number of bacteriologists who would be interested in and certainly would profit from an opportunity to discuss these problems with you.

Our own status in this work is not at all a steady one; on the enclosed card I have sent a microfilming of a draft of the ms. used at Cold Spring Harbor; we have progressed somewhat since then, but if you care to bother with a binocular microscope you will at least see how we managed to fall into this line of work. Mr. Zinder is now growing out the L-type colonies from filtrates of penicillin-treated Salmonella, and we should soon have more decisive evidence relating the following types of filtrable activity: a) prompt reversion to A form in ordinary broth; b) formation of L colonies on serum agar with subsequent reversion(?); c) transfer of genetic factors to recipient cells as discussed in the ms. The situation now is very confusing, but looks likely to clear up before long. b) and c) have not yet been separated. a) is not invariable, and can be eliminated usually by refiltration through a finer filter and by heating to 60° for 30 minutes.

For more detailed genetic work, E. coli would be more satisfactory. I have never been able to demonstrate genetic activity in cell-free preparations from strain K-12, but we're going over that ground again on the basis of the Salmonella work- so far, still no luck. The coli or aerogenes #204 that you sent was received in excellent condition, and readily showed the pleiomorphic habit that you described. The nuclear pictures are very interesting- but imperfectly shown in the attached photograph. If or when we do better, we'll send some prints on to you. Perhaps I am overwhelming you with my requests, but I wonder if you would find it possible to send the weak phage you mentioned as eliciting L-forms in strain B. We have a number of mutants in this strain (having unsuccessfully tested it for recombination) which would facilitate the necessary tests for the possible genetic role of the L-phase. If you would prefer to go into this sort of approach yourself, please let us know if there is any way that we can be of any help.— I forgot to mention that 204 has not, however, shown any signs of life in filtrates, but it will need more looking into.

It looks as if a lot of loose ends are coming together, which is quite the most rewarding aspect of scientific research.

Yours sincerely,

Joshua Lederberg